PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Association of Adverse Childhood Experiences with Diabetes in Adulthood: Results of a cross-sectional epidemiological survey in Singapore
AUTHORS	Subramaniam, Mythily; Abdin, Edimansyah; Vaingankar, Janhavi; Chang, Sherilyn; Sambasivam, Rajeswari; Jeyagurunathan, Anitha; Seow, Lee Seng Esmond; Van Dam, Rob; Chow, Wai Leng; Chong, Siow Ann

VERSION 1 – REVIEW

REVIEWER	Michael Vallis, PhD R Psych
	Family Medicine, Dalhousie University, Canada
REVIEW RETURNED	14-Dec-2020

GENERAL COMMENTS	This is an interesting paper that examines, at a population level, the relationship between adverse childhood events and diabetes, depression and resource utilisation. This paper is based on the Singapore Mental Health Study, in which 6126 interviewed citizens responded to a question about diabetes.
	This paper is generally well-written and organised. The authors do a good job jsutifying this study. While there are some data on ACEs and diabetes this has not been examined in the Singaporean population, where I was surprised to read that 63.9% report lifetime prevalence of ACEs.
	The constructs assessed are generally adequate taking into consideration that this was a population survey study. I appreciate that WHO scales were used and generally validated measures were accessed. The statistical analysis is generally well done.
	My main concerns with this paper have to do with the way the results were presented. I think, as is, it is confusing and difficult to follow. As well, there is a problem with their numbers. On page 12 we are told that 6106 were included in the analysis and 9.7% had diabetes. 9.7% of 6106 is 592. 904 is 14.8% of 6106. This is a major error. Second, the main data on diabetes and ACE is presented in a supplementary table. This is a main riding and should be not be relegated to supplemental status. Generally, the data should be more carefully presented. The 3rd paragraph pf page 13 presents critical information so quickly it is very challenging to understand it. I encourage the authors to reorganise and rewrite their results section. I do not believe that all of the readers of this journal will be able to follow these results easily.
	The discussion section is adequate but I would encourage a greater exploration of the interpretation of the risk associated with

| | the younger age. As well, the limitation of not including diabetes type should be given more attention.

This paper yields very useful information for the diabetes community. I applaud the authors for their work. |

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
This is an interesting paper that examines, at a population level, the relationship between adverse childhood events and diabetes, depression and resource utilisation. This paper is based on the Singapore Mental Health Study, in which 6126 interviewed citizens responded to a question about diabetes.

This paper is generally well-written and organised. The authors do a good job jsutifying this study. While there are some data on ACEs and diabetes this has not been examined in the Singaporean population, where I was surprised to read that 63.9% report lifetime prevalence of ACEs.

The constructs assessed are generally adequate taking into consideration that this was a population survey study. I appreciate that WHO scales were used and generally validated measures were accessed. The statistical analysis is generally well done.
We would like to thank the reviewer for the positive remarks.

My main concerns with this paper have to do with the way the results were presented. I think, as is, it is confusing and difficult to follow. As well, there is a problem with their numbers. On page 12 we are told that 6106 were included in the analysis and 9.7% had diabetes. 9.7% of 6106 is 592. 904 is 14.8% of 6106. This is a major error.
We apologise for the misunderstanding. The 'n' is the unweighted frequency while the '%" refers to weighted prevalence in the population. As the sample was disproportionate in terms of age and ethnicity the percentage in the sample varies from that in the population. Specifically, ethnic minority groups (i.e., Malays and Indians) were over-represented in our study sample and have a substantially higher prevalence of diabetes than the ethnic majority group (i.e., Chinese). As a result, weighed estimates of diabetes prevalence that give more weight to ethnic Chinese and less weight to ethnic minority groups are lower than the percentage with diabetes in our study sample. We have added a sentence in the methodology to clarify this.

Second, the main data on diabetes and ACE is presented in a supplementary table. This is a main riding and should be not be relegated to supplemental status. Generally, the data should be more carefully presented.
We apologise for this and have now included this Table in the main article.

The 3rd paragraph pf page 13 presents critical information so quickly it is very challenging to understand it. I encourage the authors to reorganise and rewrite their results section. I do not believe that all of the readers of this journal will be able to follow these results easily.
We apologise for the lack of clarity and have rewritten the results section as suggested by the reviewer.

The discussion section is adequate but I would encourage a greater exploration of the interpretation of the risk associated with the younger age.
We would like to thank the reviewer for the suggestion. We have explored this in the revised manuscript.

As well, the limitation of not including diabetes type should be given more attention.
We have elaborated on the limitation of not including diabetes type.

This paper yields very useful information for the diabetes community. I applaud the authors for their work.
Once again thank you!